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Comment on acp-2021-716

Anonymous Referee #1

Referee comment on "Trends in secondary inorganic aerosol pollution in China and its responses to emission controls of precursors in wintertime" by Fanlei Meng et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-716-RC1, 2021

The study examined annual trends in $PM_{2.5}$ chemical components based on a meta-analysis and the efficiencies of NH_3 and acid gas emission reductions on $PM_{2.5}$ mitigation. The authors also looked at hazy vs non-hazy days yet the abstract doesn't mention them – could this be addressed?

The CMAQ model run undertakes a 50% reduction in NH3 but only for January – very little comment is made of why this month was chosen and how this relates to an annual average. Comment on whether 50% reduction is a realistic target for the Chinese Government.

The authors spend a lot of time undertaking a meta analysis of the literature in order to put a database of secondary PM measurements together and this seems to have been done thoroughly, although I am not suitably familiar enough with the methods to comment further.

I don't think the CMAQ model has been evaluated for Jan 2010 using measurements of PM or PM components, although there was some evaluation of met. parameters - temperature looked good RH and especially Wind Speed were quite poor (Fig s4) – note R was 0.5 on the wind speed graph but 0.64 in the text?. There was a comparison between the CMAQ and STET model (defined as 'observations') but these were just two maps side by side. I'm not sure whether the STET model comparison is for the same period.

No evaluation of CMAQ modelled components was made either, which makes one wonder whether it did predict well in Jan 2010. Without this the conclusions are weakened somewhat. I think to have more confidence in the results more should be made of the evaluation against PM2.5 and if possible PM components.

It would have been useful for the authors to undertake a comparison of the CMAQ model predictions, associated with changing COVID emissions, and the actual measured changes.

The measurements of PM2.5 were taken using TEOM's although no mention was made of the associated problems under reading PM associated with nitrate and operational temperature, which common to these instruments. This is especially important since the paper focuses on SIA

Results

As a general comment a lot of analysis has been made between Hazy and non-Hazy days, but the conclusions and abstract don't seem to reflect this.

For the trend analysis (fig 2) suggests a 19% reduction of PM2.5 between period 1 and 3 on non-hazy days although all of the box plots are for different numbers of sites and so it would be hard to say whether this is true? also the concentrations seemed to increase in period 2? Are these trends significant?

Since the measurements are combined into periods the true trends are difficult to interpret. I think a description of a PM2.5 timeseries for a site throughout the period would be beneficial. With some comment on things like seasonality and reasons for the measurement trends. Most trends are ascribed to Government policy, although with the changes that have taken place in China, this may well be too simple

The authors mention the results in Fig 2a (page 11) and b,c,d, (page 16) which makes it hard for the reader. Consider revising the diagrams.

The authors spend quite a long time stating that PM2.5 on hazy days is greater than on non-hazy days which seems fairly obvious given that the meta analysis chose data in this way.

It says that SIA is a major influencing factor for haze pollution, yet in Fig 4 B (b) the proportion of total PM2.5 is about the same as non Hazy day 40% vs 36% respectively, suggesting that SIA goes up but so do other components of PM.

There is very little mention of the other components of PM2.5, OC,EC and the 'other' components, all of which are important – plus no model evaluation of these.

I hope these comments are useful